would have too little contact with ascertained fact to be of much value.

I will take this opportunity of mentioning an alternative theory, which has the advantage of being amenable to experimental test. If we suppose that the rate of transformation of uranium is much diminished by increase of temperature, the quantity of radium and of all the other products will be diminished too, and with it the general rate of heat production inside the earth.

The effect of heat on radium and its products has no direct bearing on the problem. Everything is governed by the primary slow transformation—that of uranium.

There is no experimental evidence on this question so far as I am aware. It could probably be best attacked by comparing the ratio of formation of uranium X at various temperatures. The amount of uranium X which had grown in the course of a few days could be determined by β-ray measurements, which might be made after cooling. R. J. STRUTT.

Sunnyside, Cambridge, February 13.

Ground Ice.

I SEE in your issue of January 30, p. 295, a letter from the Rev. John J. Hampson asking some questions on the subject of ground ice. I should like to say that my father, the late Prof. James Thomson, read a paper on this subject at the Natural History and Philosophical Society of Belfast on May 7, 1862, and I think his paper answers most of the questions. Thus he writes, after reviewing and setting aside several older theories:—"My own view is that the crystals of ice are frozen from the water at any part of the depth of the stream: whether the top, the middle, or the bottom, where cold may be introduced, either by contact or radiation; and that they may also be supplied in part by snow or otherwise: and that they are whirled about in currents and eddies until they come in contact with some fixed objects to which they can adhere, and which may perhaps be rocks or stones or may be pieces of ice accidentally jammed in crevices of the rocks or stones: or may be ground ice already grown from such a beginning.

"That pieces of ice under water have the property of adhering to one another with a continually increasing firmness, and this even when the surrounding water is above the freezing temperature, has been shown in a set of very interesting experiments by Prof. Faraday. I think too that the ready adhesion to the bottom, or to ice already anchored there, may possibly be increased by the effects of radiation, but I am confident that the anchor ice is not formed by crystallisation at the place where it is found adhering."

This paper has never been printed *in extenso*, but I hope soon to bring it out in a volume of collected papers written by my father. JAMES THOMSON.

22 Wentworth Place, Newcastle-on-Tyne,
February 11.

The Stresses in Masonry Dams.

MR. MARTIN at first asserted that my reasoning was wrong on some general principle which I failed to grasp, whereas he has now fallen back on the order of the approximation, and appeals to what he terms an axiom of practical mathematics, which he illustrates by the statement that between 0 and π a parabola can be found "differing but little from sin x." If by the method of least squares a parabola be fitted to sin x, it will be found to differ by more than 30 per cent. from the ordinate of sin x when $x=5°$; whether that difference is material or not depends entirely on what use is to be served by the correspondence.

In the memoir which has led to this controversy I showed that the equation for the stress function V, *i.e.* $\nabla^2\nabla^2\nabla^2 V=0$, was the same for a thin[1] slab and an actual dam. Since writing the paper I noticed that the third equation for the stresses was *apparently* not the same. I now see that this is only in appearance, for the terms which have a coefficient involving different functions of Poisson's ratio for the two cases are

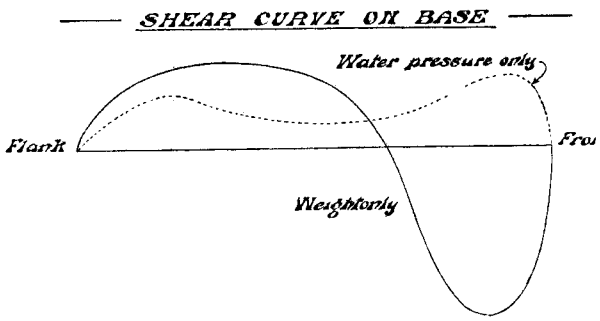
$$\left(\frac{d^2}{dx^2}+\frac{d^2}{dz^2}\right)\left(\widehat{xx}+\widehat{zz}\right)$$

and I find that this vanishes by means of the differential equation for V. Hence, as I stated in my memoir, thin plates can be used to find experimentally the stresses. Mr. Martin is therefore quite correct in his views on this point, although I cannot still agree with his demonstration of the principle.

There are, however, far more vital criticisms to be made of the memoirs recently read before the Institution of Civil Engineers than the mere question of whether the stresses in a slab and an indefinitely long dam differ by 10 per cent. or 20 per cent. A very little experimenting will suffice to show that dams when they collapse go by *stretching*, and partly at points where there may be no tension at all. The strains measured by Messrs. Gore and Wilson are not those in a real dam at all, and if we now accept the view that the stresses are the same, then we must ask Mr. Martin to allow that their stretches differ by 30 per cent. from those in an actual dam.

It was this point which I endeavoured to bring out in the criticism of the paper to which Mr. Martin has referred. If their strains correspond to those of a real dam, then their stresses differ widely; if their stresses are correct, then their strains, upon which ultimately rupture depends, will be very different from those of the actual dam. I must leave Mr. Martin to choose his own horn of the dilemma.

Again, there is another point which is physically very obvious. If a dam, reservoir empty, were split up by a series of vertical divisions parallel to its length, each plate would be of different height, and compressed under its own weight would be subjected to a different squeeze at the base of the dam. To bring these vertical sheets into contact at the correct points it is needful to suppose shear over the vertical planes at the base of the dam. In other words, there must be a distribution of shear over the base of the dam due solely to its own weight. Since the total shear over the base is zero, this distribution of shear, if the extremity of the toes be vertical, must take some such form as is shown in the diagram. Our experiments at University College showed that this base shear due to the weight of the dam only was as important as, and probably more important than, the distribution of shear due to the water pressure.



There is no evidence at all that I can see in Sir John W. Ottley and Dr. Brightmore's recent paper that they have paid attention to this point. They speak of the "original vertical lines on the model," and of measuring the displacement of these lines from "vertical lines on the glass." They speak of the return of the vertical lines on the model to the vertical lines on the glass on the removal of the water pressure. It would appear, therefore, that they have only measured the slide due to water pressure. But to deduce the stresses in the dam they must have the total shear, that due to the weight as well as that due to water pressure. I can find no evidence in their paper of any determination of the shear due to the weight of the dam. They say that the shear along the base is uniformly distributed. This, as Mr. Pollard and I showed in our memoir of last July, is *roughly*, but only roughly,

[1] Only thick plates can be properly used in dam experiments, for thin plates buckle and require a side support which destroys accuracy of experimental result. Even Messrs. Wilson and Gore's plates were at the toe as thick as they were broad.

true, if we confine our attention to water pressure. It is not true, and the base shear actually changes its sign, if the weight of the dam be taken into account as it must be. That there is no *tension* in dams of the Assuan and Vyrnwy types in the outer toe we showed in our memoir, but there is *stretch*, and on this final rupture in part depends. The existence of this stretch is also indicated, although not referred to, in the measurements of displacement given by Sir John Ottley and Dr. Brightmore. I may have misinterpreted these authors' mode of experimenting, but I can find no evidence in their paper of the manner in which they deduced the shear due to weight only, and without this knowledge I venture to think that the whole of the superstructure they base on a uniform distribution of shear fails to find any adequate foundation.

KARL PEARSON.

University College, London, W.C.

The Inheritance of "Acquired" Characters.

I AM loth to take part in barren controversy on this subject again, but I feel it is really necessary to say a few words in reply to the letter of "A. D. D." in NATURE of February 13. When that gentleman refers to the slightly dogmatic tone of his original article he is not doing himself justice; the article was as dogmatic as it could possibly be. Now he asserts that there is no sense in the distinction between acquired characters and innate ones with regard to inheritance, that all characters are both acquired and innate. This in the accepted meaning of the words is simply untrue. If a man takes a summer holiday and becomes sunburnt, the colour of his skin is an acquired character; a negro's colour develops without exposure to the sun; he is brown (not black) when he is born; that is an innate character. The supernumerary toe in a Dorking fowl is an innate character; it is not acquired in the accepted sense of that word. Acquired characters are those changes in the individual which are due to a change of external conditions, *i.e.* of stimuli; innate characters are those which develop without any stimulus, except what Dr. Reid calls the stimulus of nutrition.

When Dr. Reid says that a scar on the nose due to injury is as much innate as the nose itself he is merely quibbling; he means, I suppose, that the scar could not be formed if there was not an innate power of producing a scar in healing a wound. But the only important point is that the scar is the consequence of a wound as well as of the innate qualities; the nose is the consequence of innate qualities only. "A. D. D." appeals to Dr. Reid, but Dr. Reid has most distinctly recognised the distinction which "A. D. D." denies. They may be left to neutralise each other.

J. T. CUNNINGHAM.

Technical Research and the College System.

SINCE I made the proposal, some time ago (*Chem. News*, vol. xxxix., p. 2, and vol. xl., p. 230), that research boards should be instituted in our technical colleges, with the object of supplying the college departments with subject-matter for research of a more or less technical nature, and at the same time of keeping in touch with the old students, I have had the opportunity of discussing the matter with men who occupy important posts in the technical world. They generally hold the opinion that some such scheme is urgently needed.

Many observations are made in works and works' laboratories which for several reasons cannot be properly investigated within the factory, but are yet admirably adapted to serve as subjects for scientific investigation in the laboratories of our technical colleges. Research of such a character would be not only of real educational value to the students, but serve a special purpose in giving useful preliminary training in the investigation of problems such as they are likely to encounter in real life.

The attitude of teachers in our colleges towards such questions has been recently stated by Dr. M. O. Forster. He acknowledged that, as a teacher, it became more and more clear to him that professors ought to be educated in technology. As I previously pointed out, the suggestion, which he again brings to the front, that manufacturers should supply problems for the consideration of young chemists in the college, is one which can hardly be met in practice. The suggestion that manufacturers should supply raw materials for such trials is one that could easily be dealt with through ordinary channels if, and when, the subject-matter for research was available.

The suggestion that members of the college staff should enter the technical world for a time may be open to objection. Dr. Nichols, perhaps rightly, says that they could only return and "bring back to the students, and rehash to them, what was daily becoming obsolete." On the other hand, they would undoubtedly benefit from contact with the outside world, especially in acquiring broader ideas and in realising the way in which constant development occurs in technical processes.

I think it may be held that there is no training in our technical colleges, taken as a whole, which can compare with that given in the medical schools. Here the students actually come in contact with the work they will ultimately be engaged on in their daily routine, viz. the study of abnormal cases. Even in the departments of our more recently built colleges, which are almost small factories in their way, these necessary conditions are in the majority of cases still absent. The course simply deals with the routine work of the factory, as represented by everyday operations. This is equivalent to supplying medical students with a set of perfectly healthy men for examination, an example which well illustrates the point under consideration, for in both cases the students go out into the world to engage in actual practice. Medical men trained on these lines would hardly be tolerated by the public, yet the manufacturer is expected to receive students so trained with open arms.

To meet the conditions obtaining in the technical world, I have proposed that in every college of standing there should be constituted a research board composed of members of the staff, with possibly a few old students as advisory members.

Past students would have the opportunity to submit to these boards subject-matter for research arising out of their actual observations, and of such a nature that it can be freely investigated in a college laboratory by picked students, working under the supervision of the board. The results, if satisfactory, would be published under the names of the old and present students from the college address.

In this way colleges would be supplied with the subject-matter now so urgently needed, and the old students would be kept in touch with their college in the best possible way.

The college staff would at the same time be relieved to a great extent from the burden of supplying subject-matter of a semi-technical nature, which, to be of real value, can only be suggested by those who are acquainted with modern technical problems.

The results of some such scheme might form the basis for grants from the Government in aid of research, and also supply a rough means of testing the comparative value of the training of the colleges.

I venture to put this matter forward for discussion. It is vitally necessary that a link between the colleges in this country, and the technical world should be found. On this point everyone is agreed. I fancy that some such link may be found in the above scheme.

Occasional lectures by old students who have specialised should be given at intervals during the session with the object of interesting students in modern technical developments.

Such points as these might be brought forward, in a more prominent way, by a federation of old students' associations, which should accomplish good work in many directions.

W. P. DREAPER.

Quilter Road, Felixstowe.

A Variation in Amœba.

WHILE looking at some *Amoebae proteus* received from Mr. Thomas Bolton, I noticed a condition of the protoplasm of several specimens which I cannot remember to have seen before. The ectosarc was deeply striated, the lines extending some distance into the endosarc, wherever pseudopodia were not being put forth. On the formation of the latter the striæ disappeared, but again became